Optimum Pooling Level and Factors
Identification in Product Prototyping

by

Massimo de Falco

WP #3789-95   February 1995

Optimum Pooling Level and Factors Identification in Product Prototyping

Massimo de Falco

University of Naples "Federico II"
Dpt. of Materials and Production Engineering
P.le Tecchio, Naples 80125 - Italy

February 1995

Abstract

In the new product or process development, most of the efforts are focused on the identification of cause-effect relationships. Here, when engineers do not have the sufficient knowledge of basic phenomena, experimentation becomes the main track to obtain those linkages. Even if industrial applications typically include this practice, because of the deep statistics background required, experimentation is still ineffective and sometimes biased by human behavior.

This paper illustrates a method to support the experimenter during the analysis of effects, introducing a technique to set *a priori* the number of causes to "pool" in the residuals. This approach controls the experimental sensitivity and the risks involved in the design decisions, making the analysis more reliable and reducing, in this way, the arbitrary handling of data.

Increasing the objectivity of the methodology, the approach eliminates, as a consequence, some of the criticisms against the "pooling technique".

1 Introduction

The needs of interpreting the physical phenomena plays a main role in all development activities, particularly in the launch of new products and processes where the engineers confront problems not yet investigated. The typical approach followed in these stages are simulation and experimentation. While the first approach is effective only if we already have knowledge of the singular events composing the phenomenon and we are able to express them in algorithms, the second is inevitable if we do not have any previous information and then the only alternative is to observe the reality through experimentation.

This form of investigation is well known in the industry and already has many utilizations. However the complex statistical tools involved and the recent introduction of different oriented techniques contribute to make their correct application more confused and unclear.

Thus, the industrial needs encouraged factions of experimental design experts to develop simpler and less expensive techniques, in this way, dividing the studious into different confronting parts. One result of this new tendency is the creation of additional room for subsequent improvement.

1.1 The Experimental Design in the New Product Development

Although the experimental activity has always had a place in the industry in the Research and Development area, with the new approaches proposed by Genichi Taguchi and others, it has had a renewed powerful boost. The simplification and the shortcuts illustrated in these methodologies broke the walls of the industries and found several applications. This scenario obviously faces the inconvenience of an increased rude utilization of these methods, which already have, in themselves, criticized parts [Pignatiello J.J., 1992]. Furthermore, the quality of the experimental applications has to deal with its greatest enemy, the human nature. The aim of the experiment is purely definable as the attempt to better understand the real world through the generation of new data, but in this broad view the experimenter does not appear and his role is unraveled. The engineer or the team working on the new product development can be tempted by the desire to:
    - demonstrate the correctness of his/their intuition;

1

- push a new solution that will give him/them honors;
- study to many effects at same time;
- conclude rapidly and economically all phases.

We can face in the experiment design and conduction any of these wishes consciously or not, and their strong biasing on the result is more than evident.

This situation becomes even more critical in the complex new product development, where the numerous constraints and the expensive activity of prototype building impose a strict boundary on the number of experiments to conduct.

Besides these different sources of influence, we can identify three basic categories of behavior: the *conservative approach* in which we renounce the consideration of a new solution if there is not strong evidence of its effect; the *revolutionary approach*, where we face a greater propensity to consider the new solutions, even if we are not very confident; and the *neutral approach*, which is a more theoretical than practical concept, being very difficult to reach.

A typical conservative approach is recognizable in the aeronautic industry where, for example, the bonding assembly, even if demonstrated safe, is not allowed in the structural parts and will be excluded until everything is known about it. Differently in the consumer goods or in the electronic industry any new design is rapidly pursued to feed the market need for novelties.

1.2 Objectives of the proposed approach

This paper has as its aim the proposal of a technique to support the industrial developer in reaching a greater effectiveness in design and analysis of experiments when some constraints impose a small number of trials.

A method is illustrated to perform the "pooling" of residuals in the Anova analysis, identifying the optimum level of degree of freedom to pool in the error estimation. Thus, the pooling technique becomes less discretionary and the argued point of arbitrary handling of data is attenuated.

In the proposed procedure, the guiding criterion is the maximization of experiment sensitivity and the control of risks connected to the typical design decision making.

2

2 Risks Related to the Experimental Design Decisions

In the decision making process regarding the design solution to adopt between two alternatives, coherently with the ongoing reasoning, two different kinds of risk can be faced: the first is the possibility to accept the new design if it does not provide any improvement; the second is to refuse a new design when it actually provides an improvement. The two related mistakes, also known respectively as type I and type II errors (exhibit 1), are defined and used in the statistics hypothesis' tests, and are referred to as the null (H_o) and alternatives (H_a) hypothesis where:

H_o: The new design does not provide improvement.

H_a: The new design provides improvement.

so the decision turns in to accept or reject the null hypothesis (H_o).

Exhibit 1. Types of error occurring in the decision making.

	REALITY	
	Improvement	No Improvement
Accept New Design	OK $(1-\alpha)$	TYPE I α	Reject Ho
Refuse New Design	TYPE II β	OK $(1-\beta)$	Accept Ho

(DECISION on left axis; TEST on right axis)

Using experimental design terminology [Montgomery D.C., 1984], it is the same to say that the effects (τ_i) associated with the two levels of the design factor are not distinguishable, or:

$$H_o: \tau_1 = \tau_2 = 0; \qquad\qquad H_a: \tau_1 \neq \tau_2 \neq 0$$

where the model tested is $\eta_{in} = \mu + \tau_i + e_{in}$, η_{in} is the response observed at the i-th level and n-th observation, μ is the overall mean and e_{in} is the casual error.

3

These risks are identified by the probability of their occurrence which are commonly called α and β: P(reject H_0 | H_0 is true)=α and P(accept H_0 | H_0 is false)=β.

All these expressions change if we consider the situation where the decision must be made among more than two design alternatives in the same experiment, which means more factors levels. If we have k levels, and then k null hypotheses, we should differentiate between the probability of erroneously rejecting the null hypothesis (the already defined type I error) and the probability of erroneously rejecting at least one null hypothesis, which is called *experimentalwise error* [Mason R.L., 1989]. In fact, in this case the null Hypothesis can be written as:

$$H_0: \quad \tau_1 = \tau_2 = \ldots = \tau_K = 0;$$

the probability of occurrence of this error (E) is greater than the probability of type I, the relationship between them is expressed by:

$$E = 1 - (1 - \alpha)^{k-1}$$

which makes clear that, if only the type I error is controlled, the experimentalwise can be much larger in presence of several factor levels. For k=5, even fixing strictly α (0.05) we have E≈0.2.

2.1 The Experimental Strategy

The acceptable levels of the experimental risks are the keys to defining the outline of the strategy and the approach to follow. A conservative approach will require a low level of the α/E-error and a high level of the β-error; and vice versa for a revolutionary approach. A neutral tendency can be followed equally setting the two errors, but we should be aware that setting both errors at a low level conduces to a very unproductive experiment, while setting them at a high level could imply a very sophisticated and expensive plan.

Working in an environment with many constraints, like that which industrial engineers usually face, requires a more flexible, even if less precise, technique to perform experiments with a useful outcome. This explains the

broad diffusion that the fractional factorial design and the Taguchi method have so far obtained.

In fact, avoiding the case of unlimited resources, and therefore the willingness to perform expensive experiment, fitting the strategy and the constraints, often complicates greatly the design, making a too sophisticated pattern of the experiment necessary.

Traditionally, the industrial experimenter overcomes these difficulties by not controlling the β-error, taking risks that, tending to refuse new design solutions, penalize mostly the customers and consequently the long term firm quality.

## 2.2 Main Elements of the Experimental Design

The outcome of the experiment is entirely settled, even if not known yet, once the *framework of the product* and the *pattern of the experiment* are defined [de Falco M., 1994]. Without describing all steps in planning the experimental phase, we can identify some of the main elements, strongly interrelated, that influence the validity of the results. These are: the probabilities of the errors previously introduced (α/E, β), the sample size of each average to investigate (n), the knowledge of an initial error estimation (σ_e) and the precision of the experiment (δ). The precision is defined as the capacity to detect a change in the response of the experiment at the least of the size δ, and is usually joined with the error standard deviation in the parameter $\Phi=\delta/\sigma_e$. In this way the relationship among these parameters (appendix A) can be written, in an implicit form, as:

$$f(\alpha, \beta, \Phi, n) = 0$$

It is also possible to visualize part of this relationship in the exhibit 2, where the comparison of two normal populations with the same sample size are shown, and the area under the tails respectively at the left and at the right of the experimental observation (μ_d) represent the risk α and β; the sample size affects the curves shrinking or enlarging their shape and the precision considered $\delta=\Phi\sigma_e$ translating the curves. This shows clearly that in trying to improve the level of one parameter the others became harder to manage. For example, also illustrated in the exhibit, as imposing a greater precision ($\delta'<\delta$,

5

$\Phi' < \Phi$), without changing the other parameters, the risk β automatically increases ($\beta' > \beta$).

Exhibit 2. Relationships among the main experimental design elements.
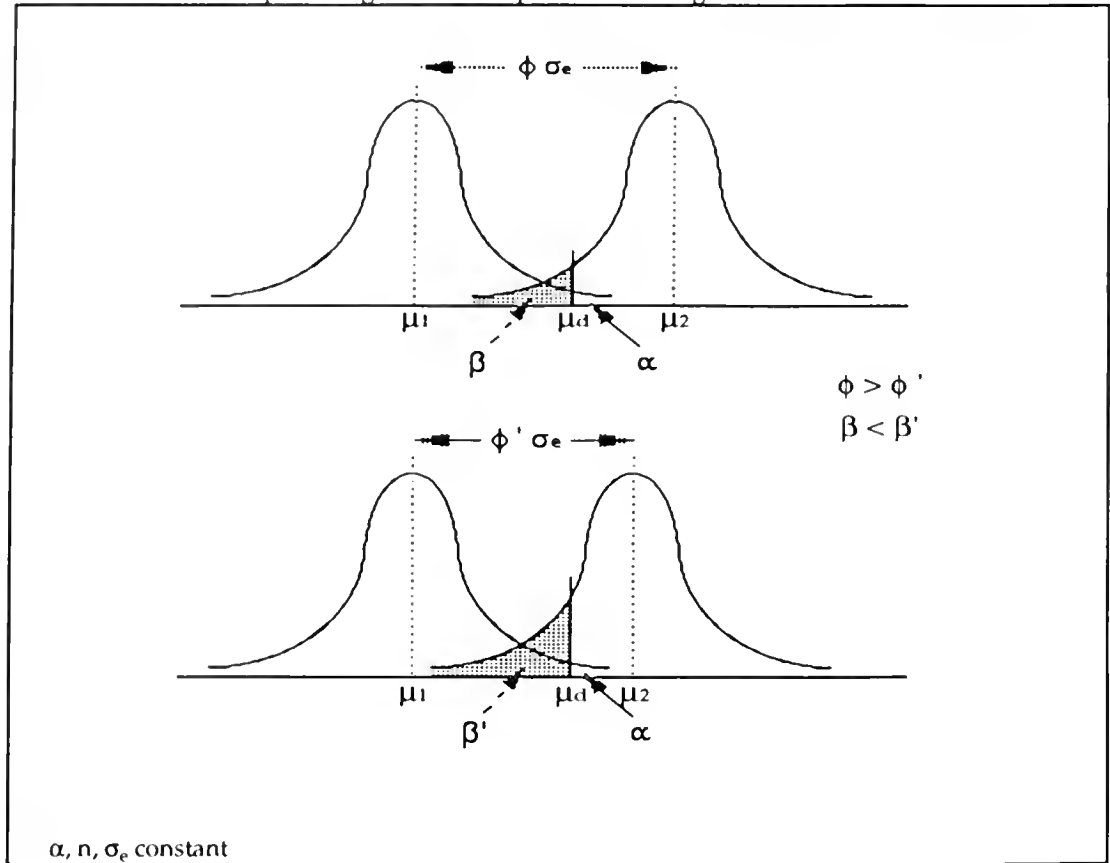
Thus, the total number of trials to run (N) is linked with the total number of levels of the factors to study (k), and the obtained sample size (n). In fact, the total number of trials comes from the product between these two parameters $N = n*k$. The number obtained must be compared with the number of factors and interactions of interest in the experiment, verifying that the necessary resolution is reached, and must be approximate to the next orthogonal array if we want to use a fractional factorial design. For experiments with different factor levels the biggest number of levels inlcuded should be used.

When the technical and economical constraints impose an upperbound for the number N, and so that the maximum number of degree of freedom available in the experiment is settled, this relationship shows how a greater number of factors/interaction implies a reduction in the resolution with the consequent confounding of the effects.

3 The proposed approach

The initial question is, how many and which factors should we include in the experiment? The answer necessarily lies on the a priori knowledge and hypotheses. In industrial application, the phenomenon under study is usually product performance, observed through a response factor, which depends on a subset of factors/interactions, included in a potential broader set of factors (exhibit 3). Here the known factors have been separated into uncontrollable and controllable, these last inlcude the "control factors" of the experiment and two remaining categories: the steady factors, which are fixed during the experiment and the variable factors, which do not have any kind of restriction.

The dependence between response and known factors is a priori representable with a cause-effect diagram, in which we delineate the relationships to test in terms of significance and relevance.

Exhibit 3. Categorization of the factors influencing the phenomenon.
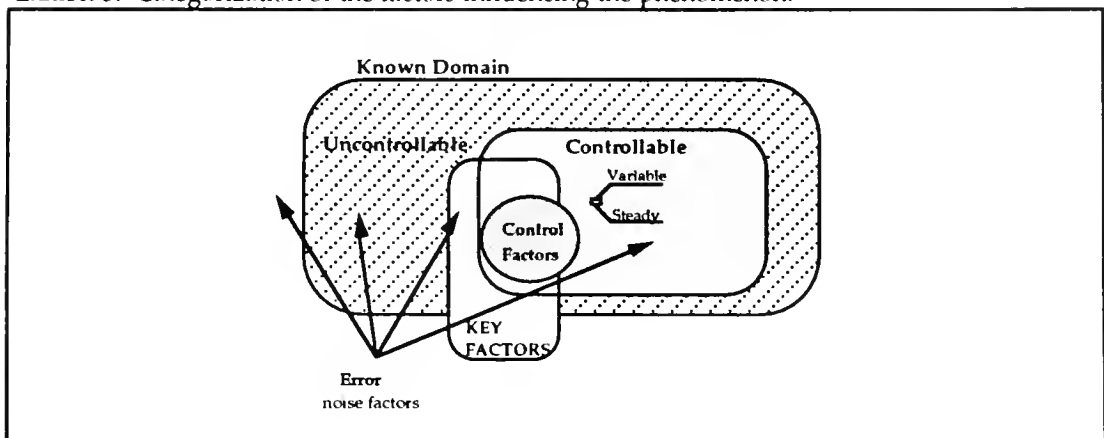
The raw knowledge in the first stage of the experimental design is the natural cause of inefficiencies. In fact, the number of factors and interactions included in the analysis could be revealed to be greater or smaller than the correct one. If the number is smaller, and there are not many way to realize that, the significance of the experiment is saved by fixing or minimizing the factors not included in it (steady factors), even if this implies an obvious tendency to neglect possible improvements of the product.

7

In more frequent cases, when more effects have been included in the model, we face, coherently with the observations of the previous paragraph, a less effective use of the experimental analysis. Here, we could ask if there is a way and, if it is correct, to intervene after the experiment to reduce this inefficiency of the initial model? This is an argued point. The traditional scientists are skeptical about any form of handling the models after the data collection, even if they do not keep always a rigid position and tolerate some forms of manipulation [Dunn O.J., 1987; Paull A.E., 1950].

The new tendency developed among the sustainers of the modern simplified industrial application of experimental design [Taguchi, Ross, Phadke, etc] do not leave doubt about this possibility and advocate, to reduce the inefficiency of the model, the use of the "Pooling Technique".

This technique relies on the principle that if a factor or interaction does not have the relevant effect initially supposed $\{\phi(C)\}$, its variation estimation is nothing more than another estimation of the casual variation, and in this sense another estimation of the experimental error (exhibit 4). Therefore, it is possible to improve the power of the test, pooling in the error the degree of freedom of this factor/interaction.

Exhibit 4. Pooling for three factors, without interactions, fixed effects analysis of variance.

SOURCE	SUM OF SQUARES	DOF	EMS
A	SS_A	a	$\sigma_e^2 + \phi(A)$
B	SS_B	b	$\sigma_e^2 + \phi(B)$
C	SS_C $\Big\}$ SS_e'	c $\Big\}$ N-a-b-1	$\sigma_e^2 + \{\phi(C)\}$ $\Big\}$ $\sigma_{e'}^2$
Residual	SS_e	N-a-b-c-1	σ_e^2
Total	SS_{tot}	N-1	

The increased number of degree of freedom in the error allows a better estimation of σ_e, and at the same time permits to keep a low level of significance in the F-test between the Mean Squares of the factor and the Mean Squares of the error ($F = MS_x/MS_e$). In fact, in this case, the degree of

freedom for the denominator increases and the F-distribution provides lower critical values.

3.1 Underlying Principles

A rule to enforce the validity of this technique can be the a priori definition of if and how to pool the factors/interactions, reducing, in this way, the form of biasing and the arbitrary handling of the data. To pursue this track we need a criterion to decide if we can pool, and a criterion to follow in deciding how and how many factors to pool.

For example, Paull suggested the rule proceed pooling if the F value obtained in a preliminary test is less than the critical value determined by $2*F_{[0.5, v1, v2]}$ [Paull A.E., 1950].

3.2 Pooling methodology

The criterion proposed here for the pooling technique is based on finding the optimum level for the error degree of freedom which reduces the probability of a type II mistake.

This is obtained by observing that, analogously to the reasoning made in paragraph 2.3 on the relationship between the precision ($\delta = \Phi \cdot \sigma_e$) and the probability β, the type II error is always linked to the capacity of the experiment to detect the change in the averages and this, if we accept the initial assumptions, becomes a function of the estimated error standard deviation.

The detection capacity will be called a posteriori "sensitivity" of the experiment and defined using the Fisher's Least Significance Difference (LSD):

$$SEN = \pm t_{[\alpha/2, v]} (2/n)^{1/2} S_e$$

here the LSD is relative to two samples of same size, and $t_{[ ]}$ is the t-distribution with an upper-tail of probability $\alpha/2$; v = degree of freedom of error; n = size of each sample compared; S_e = error standard deviation. With more samples, to control the experimentalwise error, the Bonferroni method can be adopted in the formula to assess the probability of the type I error as $\alpha/2k$, with k equal to the number of samples compared in the experiment.

9

Then the relationship between the β probability and the sensitivity can be obtained simply with the function $f[\alpha, \beta, \Phi(S_e), N] = 0$, available in table form (appendix A). This linkage allows us to draw completely the course of these parameters once the behavior of the error standard deviation is known.

<u>Error behavior</u>
The error variation is necessarily an expression of the system designed for the experiment, which is to say that the error variation depends on the *external noise* and the *experimental factors* (exhibit 5). This sources of variation in the fractional design can also be confounded with high order interactions considered negligible.

Exhibit 5.
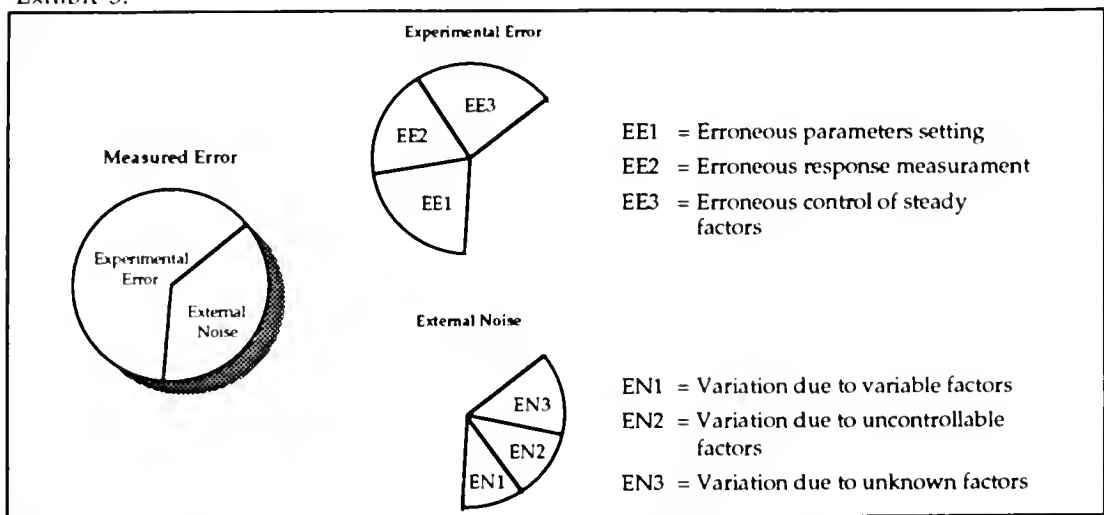
When a control factor of an experiment or an interaction under study reveals a variation lower than then error variation, pooling them together gives a smaller error variation, and vice versa. In physical interpretation, this shows how increasing the number of *small* casual sources of variation in the error allows us to smooth its estimation and to approach the normal distribution, while adding greater source of variation corresponds to including well identified sources in the error and thus the error estimation increases. This does not alterate the concept of "error", since any variation is always due to a cause, and by definition the error is the sum of the causes that we do not control. When they are many, small and not distinguishable, they give to the error the normal distribution behavior necessary for the analysis of variance.

10

Observing that an increase of error degree of freedom influences both the sensitivity of the experiment and the power of the F-test, we can draw the conceptual courses of these parameters in exhibit 6.

Exhibit 6. Experimental parameter courses.
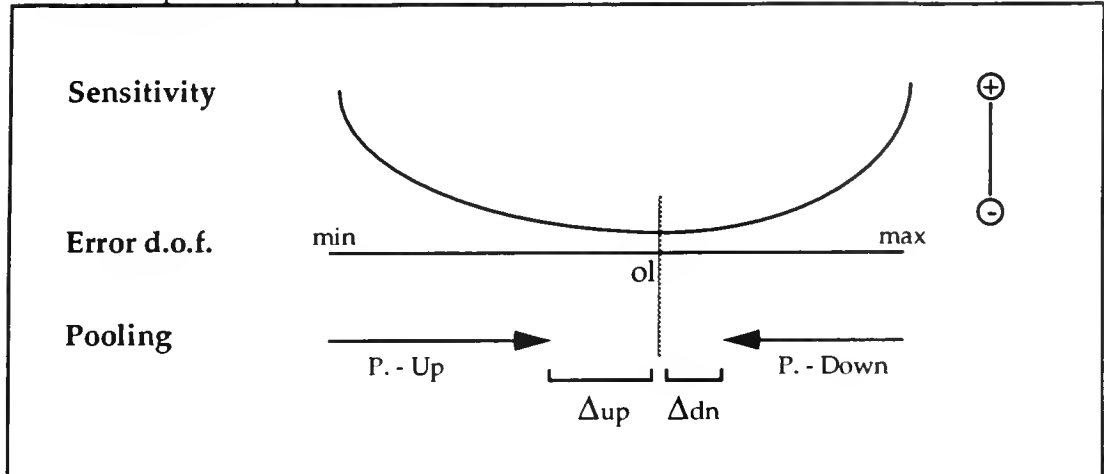
The technique proposed is to define a priori to pool the error until the dof point that maximizes the sensitivity (minimize | SEN |) of the experiment. This level of degree of freedom is a little greater than the value which minimizes the error standard deviation because the t-critical value in the formula decreases with the increasing of dof for the error.

This approach improves the p-value for the effects analyzed and at the same time reduces the probability of making the type II mistake (β mistake) approaching its minimum. This allocation of the degree of freedom optimizes the response of the experiment, keeping the analysis of the experiment more flexible in order to show possible unrevealed effects. The technique is, in this way, equivalent to the a priori assumption of high order interaction negligibility; in fact, the number of effects to include in the error is also fixed at the beginning.

Advantages of the technique

Beyond the increased capacity to distinguish the effects and the risk control, the technique makes more objective the pooling approach that is traditionally based on the subjective observation of the F-ratio obtained with the effects or the error. This avoids the possibility that the experimenter could be tempted

by the wishes initially discussed, and also represents a solution to the possible problems showed in the pooling Up and Pooling Down strategies [Ross P.J., 1988].

The first strategy tests the smallest effect against the next larger; if the ratio is not significant these are pooled to test the next one. In the Pooling Down strategy all but the larger effect are pooled; then, if the test is significant the next larger is removed from the pool and is tested with the previous effect against the remaining. In exhibit 6 we see that, in addition to the subjectivity in defining the level of significance each time, the two strategies are subject to different tendencies:

Δup= tendency to consider more significant factors;
Δdn= tendency to consider less significant factors.

These strategies both affect the type I and II mistakes, and the two tendencies constitute different kinds of effectiveness lost in the experiment. In particular, the pooling up strategy (most preferred) does not really control the related risks, because considering more significant factors, without a sustainable α level, obviously reduces the tendency to make the type II mistakes but dos not give effectiveness to the experiment.

4 The Powered Fruit Juicer application

The proposed approach will be illustrated with an application made with two Powered Fruit Juicers (PFJ). The familiarity of the reader with classic design of experiment and the anova analysis is assumed, so as to focus the attention on the subsequent steps:

i) Product Description;
ii) Sample size determination;
iii) Experiment conduction;
iv) Pooling Technique.

Product Description
The PFJs are typical consumer product diffuse in any home, mostly used for oranges, they simply work with the principle of the head rotation moved by an electric engine. The performance observed is the juice yield by the machine, in terms of the ratio between the juice produced and the juice

12

remaining in the fruit. Exhibit 7 synthesizes the phenomenon categorizing the factors related to the performance, this is coherent with the factor grouping made in paragraph 3, in which the control factors are indicated as "selected factors". This differentiation of the factors is also useful to understand how the error variation and the controlled variation are generated.

This product causes additional difficulties due to the presence of the power factor nested in the design factor (head shape), impeding, in this way, the evaluation of the interaction between them.

Exhibit 7. Factor categorization and selection.

KNOWN DOMAIN		UNKNOWN
Controllable	Uncontrollable	
Selected	Unselected		

Architecture	**Steady**	**Minimized**
• Head shape	• Lateral pressure	• Oranges dimension
• Power (Engine)	• Head dimension	• Experimental time
• Rotation technique	• Operator	

Setting	**Variable**	**Uncontrolled**
• Vertical pressure	• Rotation speed	• Orange shape
• Squeezing time	• Material	• Envir. temperature
• Axis slope	• Flash control	• Envir. umidity
	• Filter shape	

Experimental Error — External Noise

?

Architecture factor	= factor embodied in the prototypes;	Setting factor	= factor varied in the experiment;
Steady factor	= factor settled for all the experiment;	Variable factor	= factor not controlled;
Minimized factor	= factor with reduced variability;	Uncontrolled factor	= factor uncontrollable.

Sample Size Determination

This step is worth mentioning because all risks and the experimental precision are defined in this phase, any subsequent intervention has as its target only the full use of the feature defined here but not its increment. The total number of trials was calculated using tables available in literature [Davies D.L., 1956], which express the relationship $f(\alpha, \beta, \Phi, n)=0$ (appendix A), where as already introduced $\Phi = \delta/\sigma_e$ is the ratio between the precision of the experiment and the error standard deviation, n is the sample size of each

group compared in the experiment (equal, for two level factorial design, to half of the total number of trials).

In the design of experiment of PFJ, because of the total lack of knowledge, a preliminary test was conducted to estimate σ_e [Davies O.L., 1978], which came out to be approximately 0.0277. The risks α and β were both fixed at 0.05. In table A1 of appendix A, with $\Phi=1.45$ (=0.04/0.0277), we find n=14, the closest orthogonal array is n=16=N/2 and then the total number of trials will be N=32.

Experiment Conduction

The pattern of the experiment views a full factorial for the architectural factors and a half factorial for the setting factors, thus we obtained a resolution V for the architectural factors and a resolution III for the setting factors plus a resolution IV for the interactions between the factors of the two different categories, and the confounding imposed by the nested structure. The pattern is recognizable in exhibit 8 thanks to the positioning of the architectural factor on the top of the matrix, where the full factorial implies the presence of the combination of all the factors. On the side we have the setting factors where the half fractional design implies the presence of only half of the data, but the orthogonality of the design guaranties the possibility of comparing any factor at each of its two levels (Appendix C).

Exhibit 8. Matrix of experiment for the PFJ case with the results.

	ARCHITECTURE									D1	D2	D3	SETTING
A	Type I				Type II								
B	1a		2a		1b		2b						
C	1	2	1	2	1	2	1	2					
O										1	1		
B	0.73	0.81	0.65	0.65	0.44	0.62	0.39	0.55		2		1	
S	0.94	0.82	0.81	0.91	0.68	0.60	0.57	0.68		1	2		
E										2			
R	0.72	0.67	0.75	0.75	0.58	0.66	0.53	0.68		1	1		
V										2		2	
A										1	2		
T.	0.55	0.71	0.57	0.62	0.47	0.40	0.40	0.43		2			

The experiment was conducted with fixed factor level and a randomized trials technique, which means that all factors were kept at the defined levels and

the sequence of the runs was random [Montgomery D.C., 1984]. The randomization of the trials is an argued point, because, against the common perception, randomization is very expensive and the tendency to conduct the grouped runs often compromises the quality of the experiment [Pignatiello J.J., 1992].

The Pooling Technique

The analysis was started assuming negligible three factor interactions and this is already a form of effects pooling even if it does not appear in the initial Anova tables. In table 1 the initial model is shown with all second order interaction but those being relative to the nested factor, and also the p-Value is provided, which gives the smallest significance level at which the null hypothesis can be rejected. The last column supplies the percentual contribution of the effect variation with respect to the total variation and is helpful in understanding the behavior and the impact of each factor and interaction (Appendix B).

Table 1. Initial Anova table

Source	dof	SS	MS	F	p-Value	Π
A	1	0.273	0.273	54.944	0.000	42.39 %
B(A)	2	0.006	0.003	0.626	0.557	-0.59 %
C	1	0.018	0.018	3.581	0.091	2.03 %
AC	1	0.004	0.004	0.768	0.403	-0.18 %
BC	2	0.008	0.004	0.810	0.475	-0.30 %
D1	1	0.174	0.174	35.009	0.000	26.72 %
D2	1	0.000	0.000	0.013	0.912	-0.78 %
D3	1	0.060	0.060	12.073	0.007	8.70 %
AD1	1	0.002	0.002	0.349	0.569	-0.51 %
AD2	1	0.005	0.005	1.100	0.322	0.08 %
AD3	1	0.011	0.011	2.144	0.177	0.90 %
BD1	2	0.010	0.005	0.991	0.408	-0.01 %
BD2	2	0.000	0.000	0.027	0.973	-1.53 %
BD3	2	0.007	0.003	0.692	0.524	-0.48 %
CD1	1	0.005	0.005	0.912	0.365	-0.07 %
CD2	1	0.005	0.005	1.040	0.335	0.03 %
CD3	1	0.000	0.000	0.035	0.856	-0.76 %
Residual	9	0.045	0.005			24.36 %
Total	31	0.631				100.00 %

From the initial table we can start applying the pooling technique, the initial parameters are: $SEN_1 = ww$, $MS_{e1} = 0.05$ and $\beta_1 = ss$. Introducing in the error

(residual) all effects with an F-ratio less then 1, as illustrated before, the error degree of freedom became 22 and we obtained table 2. Here the parameters of the experiments are: $SEN_2=ww$, $MS_{e2}= 0.05$ and $\beta_2=ss$.

Table 2.

Source	dof	SS	MS	F	p-Value	Π
A	1	0.273	0.273	78.736	0.000	42.63 %
C	1	0.018	0.018	5.132	0.004	2.27 %
D1	1	0.174	0.174	50.169	0.000	26.96 %
D3	1	0.060	0.060	17.301	0.000	8.94 %
AD2	1	0.005	0.005	1.576	0.223	0.32 %
AD3	1	0.011	0.011	3.073	0.094	1.14 %
BD1	2	0.010	0.005	1.444	0.258	0.49 %
CD2	1	0.005	0.005	1.491	0.235	0.27 %
Residual	22	0.076	0.003			17.00 %
Total	31	0.631				100.00 %

The next step is to keep pooling in the residual the 4 dofs associated with the three smallest interactions in order to show the course of the parameters, in table 3 the results and the parameters illustrated are: $SEN_3=ww$, $MS_{e3}= 0.05$ and $\beta_3=ss$.

Table 3.

Source	dof	SS	MS	F	p-Value	Π
A	1	0.273	0.273	73.229	0.000	42.59 %
C	1	0.018	0.018	4.773	0.038	2.22 %
D1	1	0.174	0.174	46.661	0.000	26.92 %
D3	1	0.060	0.060	16.091	0.000	8.90 %
AD3	1	0.011	0.011	2.858	0.103	1.10 %
Residual	26	0.097	0.004			18.28 %
Total	31	0.631				100.00 %

This case illustrates the behavior of the experimental parameters which are also synthesized in table 4, this expresses numerically the previous courses illustrated conceptually with the curves in exhibit 6. The table also includes the value of error mean squares obtained with the preliminary 5 trials, and

the last column gives the F ratio between the actual and preliminary error mean squares (MS_e/MS_{ep}).

Table 4. Experimental Parameters in the PFJ case.

STEP	dof	MS_e	S_{EN}	F
I	9	0.0050	0.0566	6.473
II	22	0.0035	0.0434	4.517
III	26	0.0037	0.0442	4.857
Preliminary	(4)	(0.0008)		

Observing the F ratios we conclude that, except for the first step, the values are small enough to not presume any difference between the residuals and the preliminary error estimation. In addition, we should consider that in the preliminary trials we did not have the equipment setup component of the error which is responsible for part of this difference.

5 Conclusion

The proposed method is another step towards the elimination of human intervention in the experimental analysis and did not want to solve the dispute in the utilization of the pooling technique and the factors selection, but wanted to propose a track in this direction. In this sense, with the help of the PFJ application, we have illustrated the possibility of setting a priori the amount of dof to pool in the error, avoiding leaving the experimenter with the temptation to intervene with the data to fit his expectations.

6 References

Box G., Bisgaard S. Statistical Tools for Improving Designs, *Mechanical Engineering*, January 1988, p.32-41.

Bullington K. E., Hool J. N., Maghsoodloo S., A Simple Method for Obtaining Resolution IV Designs for Use with Taguchi's Orthogonal Arrays, *Journal of Quality Technology*, vol. 22, October 1990, p. 260-264.

Cochran W.G., Cox G.M., "Experimental Design", 2nd Edition, John Wiley & Sons Inc., New York, and Chapman & Hall Ltd., London, 1957.

Dehnad K., "Quality Control, Robust Design, and Taguchi Method" AT&T Bell Laboratories, Wadsworth & Brooks - Pacific Grove, California, 1988.

de Falco M., Ulrich K.T., "Integrating the Choice of Product Configuration with Parameter Design", Working Paper, Sloan School of Management - MIT, December 1994.

Desu M.M., Raghavarao D., "Sample Size Methodology", Academic Press, Inc. Harcourt Brace Jovanovich, Publ. - Boston, New York, London, 1990.

Devies O.L., "The Design and Analysis of Industrial Experiment", Longman Group Limited, New York, London, 1978.

Diamond W. J., "Practical Experimental Designs for Engineers and scientists", Lifetime Learning Publications, Belmont California - A division of Wadsworth Inc, 1981.

Dunn O.J., Clark V.A., "Applied Statistics: Analysis of Variance and Regression", 2nd Ed, John Wiley & Sons, New York, 1987.

Goh T.N., An Organizational Approach to Product Quality via Statistical Experiment Design, *International Journal of Production Economics*, vol 27, 1992, p. 167-173.

Hunter J. S., Statistical Design Applied to Product Design, *Journal of Quality Technology*, Vol. 17, N.4, October 1985.

Kacker R.N., Lagergren E.S., Filliben J.J., Taguchi's Orthogonal Arrays Are Classical Designs of Experiments, *Journal of Research of the National Standards and Technology*, vol. 96, sep-oct 1991.

Mason R.L., Gunst R.F., Hess J.L., "Statistical Design and Analysis of Experiments", John Wiley & Sons, New York, 1989.

Montgomery D. C., "Design and Analysis of experiments", 2nd Edition John Wiley & Sons, New York, 1984.

Phadke M.S., Kackar R.N., Speeney D.V., Grieco M.J., Off-Line Quality Control in Integrated Circuit Fabrication Using Experimental Design, *AT&T Technical Journal*, 1983.

Pignatiello J. J. Jr., Ramberg J.S., Top Ten Triumphs and Tragedies of Genichi Taguchi, *Quality Engineering*, 4(2), 1991-92, p. 211-225.

Ross P. J., "Taguchi Techniques for quality Engineering", McGraw-Hill, New York, 1988.

Tsui K.L., An Overview of Taguchi Method and Newly Developed Statistical Methods for Robust Design, *IIE Transactions*, vol. 5, November 1992.

Tsui K.L., Strategies for Planning Experiments Using Orthogonal Arrays and Counfounding tables, John Wiley & Sons Ltd., 1988.

Ulrich K.T., Eppinger S.D., "Product Design and Development", McGrow-Hill, Inc., New York, 1994.

Unal R., Stanley D.O., Joyner C.R., Propulsion System Design Optimization Using the Taguchi Method, *IEEE Transactions on Engineering Management*, vol. 40, August 1993, p.315-322.

Wayne A. Taylor, "Optimization & Variation Reduction in Quality", McGraw-Hill, New York, 1991.

Winer B.J., "Statistical Principles in Experimental Design", McGraw Hill, Inc. NewYork, 1970.

Appendix A

Table A1. Sample size requirements for tests on difference of two means from independent normal population, equal population standard deviations.

		Level of t-Test														
single-side		α=0.005				α=0.01				α=0.025				α=0.05			
double-side		α=0.01				α=0.02				α=0.05				α=0.1			
β =		0.01	0.05	0.1	0.2	0.01	0.05	0.1	0.2	0.01	0.05	0.1	0.2	0.01	0.05	0.1	0.2
	0.30																
	0.35																102
	0.40												100			108	78
	0.45				118				101			105	79		108	86	62
	0.50				96			106	82		106	86	64		88	70	51
	0.55			101	79		106	88	68		87	71	53	112	73	58	42
	0.60		101	85	67		90	74	58	104	74	60	45	89	61	49	36
	0.65		87	73	57	104	77	64	49	88	63	51	39	76	52	42	30
	0.70	100	75	63	50	90	66	55	43	76	55	44	34	66	45	36	26
	0.75	88	66	55	44	79	58	48	38	67	48	39	29	57	40	32	23
	0.80	77	58	49	39	70	51	43	33	59	42	34	26	50	35	28	21
	0.85	69	51	43	35	62	46	38	30	52	37	31	23	45	31	25	18
	0.90	62	46	39	31	55	41	34	27	47	34	27	21	40	28	22	16
	0.95	55	42	35	28	50	37	31	24	42	30	25	19	36	25	20	15
	1.00	50	38	32	26	45	33	28	22	38	27	23	17	33	23	18	14
	1.10	42	32	27	22	38	28	23	19	32	23	19	14	27	19	15	12
	1.20	36	27	23	18	32	24	20	16	27	20	16	12	23	16	13	10
φ=|δ|/σ	1.30	31	23	20	16	28	21	17	14	23	17	14	11	20	14	11	9
	1.40	27	20	17	14	24	18	15	12	20	15	12	10	17	12	10	8
	1.50	24	18	15	13	21	16	14	11	18	13	11	9	15	11	9	7
	1.60	21	16	14	11	19	14	12	10	16	12	10	8	14	10	8	6
	1.70	19	15	13	10	17	13	11	9	14	11	9	7	12	9	7	6
	1.80	17	13	11	10	15	12	10	8	13	10	8	6	11	8	7	5
	1.90	16	12	11	9	14	11	9	8	12	9	7	6	10	7	6	5
	2.00	14	11	10	8	13	10	9	7	11	8	7	6	9	7	6	4
	2.10	13	10	9	8	12	9	8	7	10	8	6	5	8	6	5	4
	2.20	12	10	8	7	11	9	7	6	9	7	6	5	8	6	5	4
	2.30	11	9	8	7	10	8	7	6	9	7	6	5	7	5	5	4
	2.40	11	9	8	6	10	8	7	6	8	6	5	4	7	5	4	4
	2.50	10	8	7	6	9	7	6	5	8	6	5	4	6	5	4	3
	3.00	8	6	6	5	7	6	5	4	6	5	4	4	5	4	3	
	3.50	6	5	5	4	6	5	4	4	5	4	4	3	4	3		
	4.00	6	5	4	4	5	4	4	3	4	4	3		4			

Note: The entries in this table show the number of observations needed (for each of two samples of equal size) in a test of significance of the difference between two means in order to control the probabilities of the errors of the first and second kinds at a and b respectively.

Appendix B

Percent Contribution Calculation

The variance calculated in the fixed levels experiment for a factor or interaction, listed in Anova Table, contains some amount of variation due to the error [Phadke M.S, 1983; Ross P.J., 1988]. The generic factor variance observed (V_x) can be written as:

$$V_x = V^*_x + V_e$$

where V^*_x is the variance due solely to the factor x and V_e is the variance of the error. The pure variation of factor x can be isolated as:

$$V^*_x = V_x - V_e$$

and since in the Anova Table the variance is expressed as the ratio of the sum of squares and degree of freedom of factor ($V_x = SS_x / v_x$) we have:

$$SS^*_x / v_x = SS_x / v_x - V_e$$

Solving for SS^*_x we obtain:

$$SS^*_x = SS_x - V_e \times v_x$$

then the percent contribution of the factor x (Π_x) with respect to the total variation expressed in terms of the sum of squares can be calculated as:

$$\Pi_x = SS^*_x / SS_{Tot} \times 100$$

The contribution of error (residual) is then calculated replacing all variation removed by the factors and interactions:

$$\Pi_e = (SS_x + dof_{F.I.} \times V_e) / SS_{Tot} \times 100$$

where $dof_{F.I.}$ is the total number of degree of freedom available for factors and interactions.
This correction is necessary to avoid overestimating the contribution of the effects, and it is the cause of the presence of the negative numbers in the initial Anova table.

Appendix C

Orthogonal matrix of the experiment

Table C1. Random sequence of the trials and results of the PFJ experiment.

Trial	A	B	C	D1	D2	D3	tab	J.Produced (ml)	J.Lost (ml)	Yield η (%)
1	2	2	2	2	2	2	32	51.0	67.5	0.430
2	2	2	1	1	1	2	23	56.5	49.5	0.533
3	2	2	2	1	1	2	24	61.5	28.5	0.683
4	1	1	1	2	1	1	1	85.0	31.0	0.733
5	1	1	1	1	2	1	9	118.5	7.9	0.938
6	2	1	1	1	1	2	21	81.0	58.5	0.581
7	1	2	1	2	2	2	27	62.0	46.5	0.571
8	1	2	1	2	1	1	3	89.0	47.0	0.654
9	2	1	2	1	1	2	22	68.5	35.5	0.659
10	2	1	1	2	2	2	29	55.0	62.5	0.468
11	1	1	1	1	1	2	17	97.5	38.0	0.720
12	2	2	1	2	2	2	31	47.5	72.5	0.396
13	2	2	1	2	1	1	7	49.5	76.0	0.394
14	2	2	2	2	1	1	8	70.0	57.0	0.551
15	1	2	2	1	1	2	20	115.8	38.5	0.750
16	2	1	2	1	2	1	14	76.5	50.0	0.605
17	1	2	2	2	2	2	28	86.5	53.5	0.618
18	1	2	2	2	1	1	4	85.7	46.2	0.650
19	1	1	2	2	1	1	2	90.0	21.2	0.809
20	2	1	2	2	1	1	6	66.5	41.0	0.619
21	1	2	1	1	1	2	19	80.5	27.0	0.749
22	1	1	2	1	1	2	18	91.7	46.0	0.666
23	1	2	1	1	2	1	11	86.0	20.7	0.806
24	2	1	2	2	2	2	30	48.5	73.5	0.398
25	2	2	1	1	2	1	15	67.5	50.5	0.572
26	1	1	1	2	2	2	25	73.5	59.5	0.553
27	1	1	2	2	2	2	26	93.5	39.0	0.706
28	1	1	2	1	2	1	10	110.4	25.0	0.815
29	2	2	2	1	2	1	16	67.7	32.0	0.679
30	1	2	2	1	2	1	12	110.8	10.8	0.911
31	2	1	1	1	2	1	13	77.0	35.5	0.684
32	2	1	1	2	1	1	5	51.0	64.0	0.443

tab = is the sequential order of the combination in the original matrix.

22

Date Due